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Abstract

In recent years several models have been developed in an attempt to explain countercyclical movements of job turnover, the sum of gross job creation and destruction rates. However, evidence on the behaviour over the cycle of job turnover is far from conclusive. Based on data on eight OECD countries, this paper shows that only in the US a negative and statistically significant correlation between job turnover and employment growth is observed. In the other countries, job turnover is either acyclical or mildly procyclical. Neither do institutional differences among the various labour markets -- e.g., related to the degree of "strictness" of employment security regulations -- seem to account for these asymmetries in the cyclical behaviour of job turnover between the US and the other countries. Rather than being associated to the greater flexibility of the US compared to the western European labour markets, these asymmetries in the cyclical behaviour of gross job flows have to do with statistical artifacts, namely with the little coverage offered by job turnover statistics in the US of the small business sector and with regression to the mean effects. The plan of the paper is as follows. Section 1 reviews the theoretical literature on countercyclical job flows. Section 2 provides an assessment of available data on job turnover in various countries. Section 3 presents evidence on the behaviour of job turnover rates over the cycle in eight OECD countries. Sections 4 and 5 discuss some possible explanations for the observed asymmetries in the cyclical properties of job turnover between the US and the other countries. Finally, Section 6 highlights other puzzling characteristics of job turnover that have so far received little, if any, attention by theoreticians.

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Is Job Turnover Countercyclical?

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Facts can be stylized for establishing a basis for models, but, after all, they must be facts and the way in which empirical evidence is given the status of "stylized fact" is sometimes at least debatable. Typical is the case of the literature aimed at explaining countercyclical movements of job turnover, the sum of gross job creation and destruction rates. There are today several models, which can account for a larger reallocation of jobs during recessions than during cyclical upturns. However, we will show in this paper that the posited countercyclical properties of job turnover are not adequately supported by empirical evidence. In the few cases where a negative and statistically significant correlation between aggregate employment dynamics and gross job reallocation is observed, this may be attributed to a statistical artifact, namely the regression to the mean size of the largest units in the sample.

Improving our knowledge of the behaviour of gross job flows over the cycle is important not only to refine and calibrate models, but also to assess the scope for stabilisation policies and their impact on economic restructuring. If cyclical downturns are associated with faster processes of modernisation of the economy, then fiscal and monetary stabilisation policies may also end up delaying the restructuring process. Similarly, if changes in the pace of job reallocation reflect mainly changes in the intensity of firm-specific shocks, rather than firms' responses to aggregate shocks, then demand management policies may be rather ineffective in reducing employment fluctuations.

The posited countercyclical properties of job turnover have also been used to make inferences on the behaviour over the cycle of other (often unobserved) labour market flows, like job-to-job shifts and unemployment outflows originated by the filling of positions temporarily left vacant rather than by the genuine creation of new jobs. Job turnover is defined and empirically measured as a sum of first differences in employment levels of individual establishments. Hence, the difference between labour turnover (the sum of hirings and separations) and job turnover is deemed to capture job-to-job shifts and flows from unemployment to employment associated to the reshuffling of workers across a given set of jobs.

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1 The author wishes to thank John Haltiwanger for useful comments on an initial draft. The views expressed herein are those of the author and do not necessarily coincide with those of the OECD.

However, job and labour turnover statistics typically originate from much different (and hardly comparable) data sources. Moreover, there is evidence that a significant component of job turnover is associated with temporary and mean-reverting changes in the size of business units. In other words, job turnover also captures many transient establishment-level employment variations most likely occurring as a result of voluntary separations and associated vacancy chains. Hence, even if job turnover was found to behave countercyclically, this finding could hardly be used to make inferences on the cyclical properties of worker flows associated (or complementary) to job turnover. Fortunately. new data sources are being developed which will provide information on both job and worker flows. Until such data become available, it may preferable not to overstate the heuristic content of job turnover statistics in the understanding of the time profile and cyclical properties of gross worker flows.

The plan of the paper is as follows. Section 1 reviews the theoretical literature on countercyclical job turnover. Section 2 provides an assessment of available data on job turnover in the various countries. Section 3 presents evidence on the behaviour of job turnover rates over the cycle. Sections 4 and 5 provide some possible explanations for the observation of countercyclical job turnover in the US and procyclical or acyclical in the other countries. Finally, Section 6 highlights other rather puzzling characteristics of job turnover --whose explanation could considerably improve our understanding of the job generation process-- that have received so far little, if any, attention by theoreticians.

1. The models....

We have counted at least five models developed in the most recent years in an attempt to explain countercyclical movements of job turnover.

In the model developed by Davis and Haltiwanger (1990) job creation is time-consuming while job destruction is not. Fixed time-costs of moving from one job to another and/or making the new job operational[2] smooth out the response of job creation to contemporaneous shocks. As a result, job destruction fluctuates more over the cycle than job creation. Because of these "reallocation-timing" effects, cyclical upturns involve more falls in job destruction than increases in job creation (hence reductions in job turnover), while downturns involve a rise in job destruction and only a moderate fall in job destruction (hence larger gross job reallocation).

2 As suggested by Davis and Haltiwanger (1990), a rationale for these fixed time effects is the time spent either while moving between two different production sites or while opening a new plant, and/or investing in match-specific capital.

The model developed by Caballero and Hammour (1992) draws on an initial intuition by Blanchard and Diamond (1989): recessions are times of "cleansing" where creative destruction processes occur. Downturns cannot be accommodated mainly by changes in the pace at which new jobs are created --as opposed to changes in the magnitude of job destruction-- because fast creation of jobs in an industry[3] is costly due possibly to congestion effects in the industry's labour market. Alternatively, job creation only partly "insulates" job destruction --that is, declines in demand are only partly accommodated via reductions in job creation rates rather than larger job destruction rates-- during downturns because the opportunity cost of starting-up a new enterprise is lower during downturns than in periods of economic recovery [Blanchard and Diamond, 1990]. In both cases there is more time variation on job destruction than in job creation, and gross and net job changes are negatively correlated.

In both Davis and Haltiwanger's and Caballero and Hammour's models, countercyclical movements of job turnover are ultimately a byproduct of costs in creating new jobs while job destruction is costlessy and instantaneous. Another rationale for the posited asymmetry between gross job creation and destruction is in the costs associated with the filling of vacancies. In other words, job creation is time-consuming because it takes time to establish a match with some positive surplus. This is the route pursued by Mortensen and Pissarides[4] (1994): in their job-search model, countercyclical movements of job turnover are originated by the time required to fill vacancies opened during upturns. During downturns, jobs are destroyed and vacancies are cancelled immediately. These asymmetries are even stronger when cyclical shocks are anticipated by firms and workers and when cyclical shocks have a low degree of persistence.

Mortensen and Pissarides' model draws ultimately on the distinction between posting a vacancy and creating a job. Cyclical upturns affect immediately unfilled jobs, but have little immediate effect on actual job creation. Hence, job turnover is countercyclical. Similar is the approach followed by Burda and Wyplosz (1994) who distinguish between vacancies associated to unfilled jobs and vacancies associated to the creation of new positions. In Burda and Wyplosz's model, the size of the shock hitting firms during recessions is crucial. For low values of the shock, firms can simply cancel planned positions or close unfilled vacancies, rather than dismissing workers. For high values of the shock, the opportunity cost of dismissing workers is lower than the cost of keeping vacancies unfilled, hence layoffs become the dominant form of employment adjustment. Large number of

3 Plant-level adjustment costs *per se* would not necessarily smooth job creation at the aggregate level. As shown in Caballero (1992), micro-level asymmetries do not necessarily carry-over to macro data.

4 Pissarides and Mortensen are more cautious than other authors in presenting empirical evidence on countercyclical movements of job turnover. In fact, they claim that "empirical evidence on the cyclical issue is inconclusive" (Pissarides and Mortensen, 1994, p.412).

workers released during recessions in turn increase the number of "good" workers in the unemployment pool, stimulating the filling of other vacancies, thereby contributing to reduce the negative impact of recessions on gross job creation.

Finally, Burgess (1991) and Blanchflower and Burgess (1993) extend traditional search-theoretic models by allowing for job search by the employed. Countercyclical job reallocation and procyclical labour turnover are in this context accommodated by large and strongly pro-cyclical job-to-job shifts.

2. ...the data

Most data on job turnover used in empirical work come from administrative sources. In general, social security and unemployment insurance records (as in the case of France, Germany and Italy) or tax forms filled in by employers (as in the case of Canada, Denmark, Norway and Sweden) provide information on the number of full-time employees in individual business units at annual frequencies. This makes it possible to measure job turnover as a sum of establishment-level employment changes. It should be stressed that jobs are, in this context, *filled* positions. Ceteris paribus, if an individual leaves an establishment and, at the time of the statistical recording, has not yet been replaced, this unfilled job will be counted as a job loss. In other words, empirical measures of job turnover tend to overstate the actual turnover of posts, whether filled or unfilled. Conversely, job turnover is likely to seriously understate labour turnover, that is, the sum of gross hirings and separations over business units. As individual posts within an establishment cannot be identified by data, hires and separations are in fact not counted which net out at the level of an individual business unit.

The above holds also for data on job turnover collected on the basis of establishment surveys (as in the case of the LRD data used in most studies on the US). An advantage of survey data with respect to information drawn from administrative sources is that they may be available at higher (e.g., quarterly) frequencies and that their coverage tends not to be affected by changes in regulations. By the same token, they seem to provide a better basis for international comparisons insofar as differences in the coverage of social security schemes or in the degree of misreporting of tax information should not affect survey data. However, unlike most administrative data, business surveys usually involve only enterprises above a given threshold[5] and the survey panel is usually renewed only at discrete intervals, which reduces the representativity of the sample the further from the rotation date and makes it more problematic the measurement of entry

5 The high costs of surveys often suggest introducing relatively large thresholds in the size of business units that end-up reducing dramatically the sampling frame. Small units are also excluded from surveys in order to reduce respondent burden for small employers.

and exit[6]. In the case of the LRD data, for instance, only enterprises with more than five employees are surveyed, which leaves aside a large component of job turnover, namely job turnover originated by entry and exit of firms or establishments in the fringe.

The statistical unit varies across countries as well as recording and de-recording procedures. In Germany, for instance, the unit is the establishment or the plant, identifiers do not change in the case of mergers or acquisitions, and hence only "true" births or deaths are counted. By contrast, in Sweden not only ownership changes, but also changes in the staff of any individual plant involve the attribution of a new ID number. In other countries, such as Canada and the US, it is possible to have information on both plant openings and closures and ownership changes for continuing establishments as different identifiers are assigned for the ownership and location of plants.

Given these differences in units of measurement and coverage of national series, cross-country comparisons may not be particularly meaningful[7]. For this reason, in the remainder, we will confine ourselves to analysing the time-series properties of job turnover in each country.

3. ..and the facts

Table 1 displays correlation coefficients between job turnover and aggregate employment growth for all countries in which data on gross job flows were available for sufficiently long a time period to make this analysis meaningful[8]. As shown in Chart 1, all countries selected have experienced both expansionary periods and cyclical downturns during the observation period.

Job turnover is in Table 1 defined as follows:

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6 In fact, there is a high risk of mistakenly imputing to entry and exit the rotations of the panel.

7 See, however, OECD (1987) and OECD (1994) for comparisons of job turnover rates across countries. See also Blanchflower (1994) and Grey (1993) for a discussion of cross-country comparability issues.

8 Data were collected by OECD aggregated by industry and, limited to some countries, by establishment size classes. See the Annex for details on the national data sources and OECD (1994) for a cross-industry analysis of job turnover rates.

Table 1
The Time-Series Relation between Net and Gross Job Flows

(Rank correlation coefficients(1), yearly data)

Country	Year	$\rho(NET_t, JT_t)$	$\overline{JT}$	σ_{JT}
Canada	(1978-90)	.00 (1.00)	27.5	2.5
Denmark	(1980-89)	-.08 (.81)	29.4	1.1
France	(1978-88)	.72 (.03)	24.4	2.3
Germany	(1977-90)	-.08 (.78)	15.9	.6
Italy	(1984-93)	.30 (.36)	23.0	1.3
Norway(2)	(1976-86)	-.14 (.68)	14.9	1.5
Sweden	(1985-92)	.43 (.29)	29.1	1.1
US(2)	(1973-88)	-.43 (.10)	19.4	2.0

Sources: See the annex.
Notes:
(1) Spearman rank-correlation coefficients; marginal significance levels in parentheses.
(2) Manufacturing only.

$$JT_t(E) = \frac{\sum_{i \in E} | x_{it} - x_{it-1} |}{\sum_{i \in E} x_{it-1}} \qquad (1)$$

where x_{it} denotes the number of employees of any establishment i at time t. Thus employees in exiting units are counted in the denominator of (1) whilst employees in units created between t-1 and t are included in the numerator[9]. Net employment growth (NET) is given by:

$$NET_t(E) = \frac{\sum_{i \in E} (x_{it} - x_{it-1})}{\sum_{i \in E} x_{it-1}} \tag{2}$$

Strikingly enough, in all countries, except the US, the correlation coefficient between gross and net job flows is either positive or statistically insignificant[10]. In other words, there is no evidence of countercyclical movements of job turnover, at least outside the US. In the latter case the correlation between JT and NET is negative and significant at 90 per cent confidence levels, as reported in previous studies by Davis and Haltiwanger (1990 and 1992). The US is also one of the countries with the lowest magnitude and largest time-series variation of job reallocation rates (second and third columns of Table 1), but comparisons of this kind are problematic given the data issues discussed above and the fact that available data cover different time-periods and cyclical conditions in the various countries.

4. Why is the US so Different?

A possible explanation for the observed asymmetries in the cyclical behaviour of JT between the US and the other countries could rely in institutional differences between North-American and Western European labour markets. In particular, the greater flexibility of the US and Canadian labour markets is often stressed, in contrast with tight dismissal regulations prevailing in most European labour markets. The costs incurred by employers in laying-off workers may smooth over time the accelerations in gross job destruction occurring during downturns, thereby reducing the correlation between JT and NET. Hence, a lower degree of

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9 An alternative weighting of gross job flows was suggested by Davis and Haltiwanger (1990) which uses as denominator in (1) the average between employment in the base and in the final year, and hence bounds growth rates of exiting units and entrants between -2 and 2. This alternative (perhaps less intuitive) measure of job turnover would not affect our results. Entry and exit processes seem in fact to play a minor role in the cyclical behaviour of JT and NET (Boeri and Cramer, 1992; OECD, 1994).

10 Spearman rank correlation coefficients are reported in the table rather than simple correlations insofar as limitations in the coverage of job turnover series would suggest not having too much confidence in the scale of the two covariates. In any event, results obtained while computing Pearson correlation coefficients were not different: the US was also in this case the only country in which we observed a statistically significant and negative correlation between JT and NET.

responsiveness of gross job destruction to cyclical conditions should be observed in heavily regulated European labour markets compared to the US and Canadian case[11].

The simple model developed in Annex 2 helps characterising the effects of employment security (ES) schemes on gross job flows. It suggests, first of all, that higher firing costs tend to reduce aggregate job flows. This is at odds with table 1, which points to relatively high job turnover rates in highly regulated labour markets like Italy and France and low JT in "flexible" labour markets like the US. A second implication of the model is that the tightening of ES schemes increases the cyclical responsiveness of gross job creation (POS) relative to job destruction (NEG), thereby contributing to partly "insulate" NEG from aggregate shocks. Finally, the model suggests that also net employment flows become less responsive to aggregate shocks as a result of tight dismissal regulations.

Table 2 reports results obtained by pooling time-series and cross-section observations and regressing net and gross job flows against the rate of growth of GDP in three different groups of countries capturing the institutional differences outlined above. In particular, group 1 includes the two North-American labour markets in our database (US and Canada), group 2 denotes the Nordic countries, namely Denmark, Norway and Sweden, whilst group 3 is composed of France, Germany and Italy. These three regional groupings roughly correspond to different degrees of tightness of employment security regulations. Several rankings of countries based on the "strictness" of employment protection schemes have been defined in recent years[12], in which the US and Canada typically feature among the countries with fairly extensive freedom to dismiss, the Nordics occupy intermediate positions being generally more restrictive in notice and severance pay regulations than in procedural obstacles to the implementation of no-fault dismissals, while Germany, France and Italy are generally considered as having "fundamental" employment protection constraints in all domains[13].

Given the large differences in the magnitude of JT in the various countries documented above (which do not seem to be associated to varying degrees of ES and can be attributed to the different coverage of job turnover statistics in the various countries, as discussed in the previous section) we preferred to focus on the time-series association between job flows and GDP growth only. Thus, we display within-groups estimators only. Under this specification, country-specific fixed effects are allowed for, but common slope coefficients are imposed across

11 See Baldwin, Dunne and Haltiwanger (1992) for a comparison of US and Canadian job reallocation rates. Consistently with our findings, the authors did not observe countercyclical movements of job turnover in Canada.

12 See OECD (1994), Bertola (1990), IOE (1988) and Grubb and Wells (1993).

13 See, in particular, IOE (International Organisation of Employers), ibidem.

the various countries. Hence, we allowed for varying regional elasticities of job flows to GDP growth and tested the homogeneity of the coefficients across the three regional groups described above.

Table 2

Employment Security Schemes and the Cyclical Properties of Job Turn-over

(within-groups regression results)

Dependent variable	nobs	GDP	GDP_2(1)	GDP_3(2)	Wald	Wald R(3)
NET	80	1.20	-.24	-.28	1536**	2.64
		(38.77)**	(.69)	(1.48)	(3)	(2)
POS	80	.52	.08	.00	573**	.41
		(23.10)**	(.13)	(.00)	(3)	(2)
NEG	80	-.67	.30	.28	174**	6.41*
		(12.56)**	(1.36)	(2.26)	(3)	(2)

Notes: Robust t-statistics and degrees of freedom (for Wald tests) are displayed in parentheses. One asterisk denotes significance at 95, two asterisks at 99.
(1) GDP coefficient (expressed in terms of deviation from the slope coefficient for the US and Canada) for the group of Nordic countries (Denmark, Norway and Sweden).
(2) GDP coefficient (expressed in terms of deviation from the slope coefficient for the US and Canada) for the group of Central European countries (Germany, France and Italy)
(3) Wald test of joint significance of the two region-specific coefficients.

Sources: See the Annex.

The sign of the coefficients is in line with a-priori expectations: NET and gross job creation increase during upturns whilst gross job destruction moves countercyclically. However, we found little support to the hypothesis that ES regulations affect the cyclical responsiveness of gross job flows. Country-specific slope coefficients are, in fact, not significant, except in the case of the elasticity of gross job destruction which appears to be significantly lower in Germany, France and Italy than in the US and Canada. Similar results were obtained by running the model against lagged GDP growth[14].

14 Given the low numbers of observations available in each country it was not possible to run regressions including both simultaneous and lagged GDP growth.

Although longer time-series would be required to better assess the effects of employment protection schemes on the cyclical properties of job turnover[15], the above does not lend support to the view that asymmetries in the cyclical behaviour of JT in the US compared with the other countries can be attributed to institutional differences among the various labour markets. Employment security schemes are often difficult to enforce, and there are many ways employers can reduce the workforce while avoiding procedural complications and dismissal costs. For instance, there is ample evidence suggesting that employers react to tight employment security schemes by inducing a larger number of workers to quit [Burgess and Nickell, 1989]. Furthermore, employment security schemes may be a source of fixed or "lumpy" adjustment costs which result in much different a time profile (and one generally increasing, rather than reducing the time variation of NEG) for layoff policies of firms than in the standard case of quadratic adjustment costs (Hamermesh, 1993).

The degree of unionisation of the workforce may be more relevant than employment security regulations in accounting for observed asymmetries in the cyclical behaviour of JT. However, there is no evidence that the degree of unionisation of the workforce can explain the wide cross-industry variation in job turnover rates observed in countries like the US [Davis, Haltiwanger and Schuh, 1994] and highly unionised countries, like the Nordics, tend to display the largest job reallocation rates (Table 1).

5. Cyclical Properties of JT and Regression to the Mean Fallacies

Data issues are another obvious candidate for explaining the differences between the US and the other countries reported in Table 1, and hence we turn next to these issues.

US data on job turnover differ from those of the other countries in at least three important respects. First, while data on Canada, Denmark, France, western Germany, Italy, Sweden and Norway come from administrative sources, US data are drawn from a sub-sample of the Annual Survey of Manufacturing (see the Annex for details on national sources). Secondly, US data refer only to the manufacturing sector whilst elsewhere they tend to cover not only manufacturing,

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15 Longer time-series would also permit to characterise patterns of autocorrelations in job creation and destruction, and hence better assess the degree of "smoothing" of job destruction associated with tight employment security schemes. First order autocorrelations in job destruction computed on the basis of available data are positive and significant in the case of Canada, Denmark and Germany and are insignificant elsewhere. Explanations of asymmetries in the cyclical behaviour of job turnover based on the degree of tightness of employment protection schemes would suggest that the autocorrelation should be stronger in the countries with the highest levels of protection (e.g., Italy and France). But the limited number of observations available place serious constraints on the interpretation of these results.

but also services[16]. Thirdly, US data cover only establishments with more than 5 employees and as the sampling frame is renewed only at Census years frequencies, there is also a high risk of undersampling small units above that threshold the further away from the last Census and the stronger the growth of the small business sector between any two Census years[17]. And there is no indications of countercyclical movements of job turnover when other job flow data for the US are used, which offer a better coverage of the small business sector. For instance, the correlation coefficient between JT and NET computed on the basis of USEEM data[18] referred to the period 1976-91 was -.2 which is not significant at conventional levels.

Sample selection against small units (if not the underreporting of small business) may contribute to explaining differences in cyclical properties of job turnover between the US and the other countries. In fact, the exclusion of the smallest units seriously biases job turnover data against gross job creation. Although plants with less than five employees generally account for no more than 5 per cent of total employment, they generate a very large component of total gross job flows[19]. As vividly documented by all the most recent empirical literature on the relation between growth and size of firms[20], net job creation is highly concentrated among small and young business units whereas gross job destruction usually exceeds gross job creation when the focus is on the largest business units[21].

16 Data on Norway do not cover services, but only industry, that is manufacturing plus mining and quarrying. As discussed below, this may contribute to explaining the negative (although not statistically significant) correlation coefficient for this country.

17 Data from the US Small Business Administration commented in OECD (1994) suggest that the employment share in establishments with less than 20 employees would have indeed increased considerably at the turn of the eighties.

18 In the US Establishment and Employment Microdata (USEEM) the average size of plants is 17.7 compared with 80 in the LRD sample used by Davis and Haltiwanger. The USEEM panel --like most administrative data sources-- covers all establishments with at least one employee, but requires also that units have a Dun & Bradstreet credit rating for inclusion in the dataset. Unfortunately, we had no access to USEEM JT data by establishment size classes, which would have also been useful to assess the role played by sample selection in the asymmetries between LRD and USEEM data.

19 In western Germany, for instance, gross job flows generated by plants with less than 5 employees accounted for more than 50 per cent of total job turnover in the period covered by data [Boeri, 1994]. Similarly, in Denmark, Italy and Sweden, plants with less than 20 employees accounted for between 55 and 66 per cent of total gross job creations [OECD, 1994].

20 As shown in Boeri (1989), all the most recent literature on the testing of Gibrat's Law rejected the presence of proportionate effects in firms' growth unlike the studies carried out in the 1960s and 1970s. A possible explanation for these conflicting results is in the little coverage provided by earlier studies of the small business sector. A strong negative relationship between size and growth has been observed especially in the case of the smallest units.

21 In Canada, Denmark and Sweden, *net* job creation among units with less than 20 employees was, in the period covered by data, above 2 per cent, compared with -.3 to -.6 per cent net employment declines in units with more than 500 employees [OECD, 1994].

Similarly, focussing only on manufacturing units means understating economy-wide gross job creation relative to gross job destruction insofar as in all countries the employment share of manufacturing has been steadily declining in the period covered by data.

Summarising, sample selection against small units or the service sector tends to reduce the magnitude, hence the variance, of job creation relative to job destruction rates. By reducing the time-series variation of gross job creation relative to gross job destruction, the neglect of small units may bias downwards correlation coefficients between job turnover and net employment growth[22].

Table 3
Time-Series Variation of Gross Job Creation and Destruction

Country	Var(POS)	Var(NEG)	CV(POS)	CV(NEG)
Canada	5.35	8.19	.15	.22
Denmark	.50	.54	.08	.08
France	2.84	.80	.14	.07
Germany	.72	.71	.10	.11
Italy	1.42	.74	.10	.08
Norway(1)	1.29	1.82	.16	.17
Sweden	3.99	2.23	.14	.10
US(1)	4.08	8.96	.22	.29

Sources: See the annex.
Notes:
POS and NEG as defined in the text.
CV stands for the coefficient of variation.
(1) Manufacturing only.

22 Denoting by POS gross job creation (the sum of establishment-level employment changes among expanding units and new entrants) and by NEG gross job destruction (the sum of establishment-level employment changes among declining and exiting units), we have that:

$$Cov(JT,NET) = Var(POS) - Var(NEG)$$

because $JT \equiv POS + NEG$ whereas $NET \equiv POS - NEG$. Denote then the true values of POS and NEG by an asterisk and suppose that the measured job creation and destruction rates are a constant fraction of the true values, i.e., that:

$$POS = \mu POS^* \qquad NEG = \gamma NEG^* \qquad 1 > \gamma > \mu > 0$$

where we have taken into account the fact that small units contribute more to gross job creation than to gross job destruction. It then follows that:

Table 3 collects available evidence on the time-series variation of gross job creation (POS) and destruction (NEG) rates. By comparing variances (first and second column) with the coefficients of variation (third and fourth column) it is possible to grasp the role played by scale effects in increasing the variance of NEG relative to POS and hence in altering the cyclical properties of job turnover. In Norway, for instance, the variance of NEG is larger than the variance of POS, and therefore the covariance between JT and NET is negative. However, when the two measures of dispersion are properly standardised (third and fourth columns), it turns out that POS varies more over time than NEG. Similarly, in the US the variance of job destruction is much larger than the variance of job creation, whilst differences in the coefficients of variation of POS and NEG are of a second order magnitude. The case of Italy is symmetric: here scale effects tend to magnify the time variation of POS relative to NEG, thereby inducing a positive correlation between JT and NET.

The role played by sample selection in the correlation between net and gross job flows can also be assessed by comparing correlation coefficients between JT and NET for small and large units and for plants in industry and services. Limited to three countries representative of the broad regional groupings outlined above --Canada, Germany and Denmark-- we had indeed access to data on job turnover by sector and by size of establishments.

Table 4 displays correlation coefficients between JT and NET for different sectors and for the smallest (less than 20 employees) and largest (more than 500 employees) establishments in Canada, Germany and Denmark. Strikingly enough, small units seem to exhibit some procyclical pattern (especially in the case of Canada), whilst large units display negative correlation coefficients[23]. A negative correlation between JT and NET is also observed in manufacturing, but the coefficients are not statistically significant. Job turnover in services would seem to be in all countries either acyclical or mildly procyclical. It is difficult to disentangle firm-size from industry effects on the cyclical properties of job turnover. Insofar as most small business units belong to the service sector, the different cyclical properties of job turnover in services and manufacturing may indeed be merely a byproduct of firm-size effects. In any event, whether due to regression

23 Consistently with our findings, Violante and Prat (1992) report positive correlation coefficients for small units and negative for large firms in Italy. Davis, Haltiwanger and Schuh also found that even in US manufacturing job turnover in small, younger, less specialised and lower-wage plants does not behave countercyclically. Insofar as young and highly specialised plants are smaller than the average and that there is a strong positive correlation between firm size and wages, this finding may also point to firm-size (regression to the mean) effects. Konings (1993) computed correlation coefficients between job turnover and net employment change for a sample of large UK firms (averaging about 4,000 employees) observing a negative correlation between JT and NET like that observed in Canada, Germany and Denmark for the largest plants. Finally, Broersma and Gautier (1995) display positive correlations between JT and NET for plants with less than 100 employees and negative for the largest units in the Netherlands in the period 1979-91.

Table 4
Industry and size of plants and the cyclical properties of job turnover

(Rank-correlation coefficients)

	Canada	Germany	Denmark
All sectors	.00 (1.00)	-.08 (.78)	-.08 (.81)
Services	.09 (.75)	.14 (.65)	.00 (1.00)
Manufacturing	-.09 (.76)	-.38 (.23)	-.40 (.26)
Small units(1)	.64 (.03)	-.04 (.90)	.06 (.85)
Large units(2)	-.28 (.33)	-.50 (.10)	-.15 (.67)

Sources: See the Annex.
Notes: Spearman rank-correlation coefficients; marginal significance levels in parentheses.
(1) Establishments with less than 20 employees.
(2) Establishments with more than 500 employees.

to the mean[24] of the largest units or to the declining trend of employment in manufacturing, the posited countercyclical properties of job turnover look very much like an unsurprising statistical artifact.

6. Job and Worker Flows

The above suggests that, in spite of the several models produced in recent years to account for counter-cyclical movements of job turnover, evidence on the cyclical properties of gross job reallocation is far from conclusive. Only gross job flows originated by changes in the size of the largest establishments or firms would appear to behave countercyclically in all countries, but this is likely to reflect a mean-reverting process in growth --the fact that the largest units have a higher probability than smaller units to decline-- rather than important asymmetries between gross job creation and destruction like those framed by the theoretical literature. Moreover, these "countercyclical" components of job turnover account

24 Plants that are large in one year are more likely than small units to have experienced a shock that pushed them to expand capacity and that will be reverted next year. Similarly, small plants are more likely than large units to have recently experienced a transient negative shock.

only for a very minor component of total gross job flows. To give an example, in Germany the largest establishments (those with more than 500 employees) accounted over the 1980s for 30 per cent of total employment, but for only about 5 per cent of total of job turnover [Boeri, 1994]. Hence, to the extent that some components of job turnover appear to be negatively correlated with employment growth, they represent only a very minor segment of total gross job flows.

In addition to providing material for model-builders, the cyclical properties of job turnover have also been used to make inference on the behaviour over the cycle of labour market flows on which little, if any, information is generally available. A typical example is that of job-to-job shifts. As stressed by Davis, Haltiwanger and Schuh (1994), procyclical labour turnover together with countercyclical job turnover implies that the component of labour turnover which is not associated to job turnover, being mainly the result of the reshuffling of workers across a given set of jobs, is procyclical.

The "stylized fact" challenged in this paper has also been used to draw conclusions on the main factors lying behind the time-series variation of unemployment inflows and outflows[25]. Evidence from various countries suggests that unemployment flows move countercyclically and that flows to and from out-of-the labour force display little cyclical variation. In other words, the pace of flows between employment and unemployment (the so-called E-U flows) would seem to drive the behaviour of unemployment inflows and outflows over the cycle. Against this background, countercyclical job turnover (together with procyclical labour turnover) would seem to imply that the time-series variation of unemployment inflows and outflows is mainly associated with changes in the pace of gross job creation and destruction rather than with changes in the intensity of worker reallocations across a given set of jobs.

There are, in our view, a number of problems with this use of job turnover data in arguing about the cyclical properties of unobserved worker flows. The first problem is that job turnover and labour turnover (LT henceforth) statistics are not comparable as they generally come from different sources and use different units and methods of measurement. Job turnover counts jobs, while the statistical units of labour turnover are the individuals. Job turnover is a discrete time measure, while labour turnover is not. Job turnover is, in fact, measured by taking first-differences of employment stocks, while labour turnover records all hirings and separations occurred within a given time-period. Finally, available data on job turnover do not allow to distinguish individual jobs within establishments. Hence, changes in the characteristics of jobs offered by any establishments are not recorded when these do not involve changes in the size of business units.

25 See Davis, Haltiwanger and Schuh (1994), Chapter V and Burda and Wyplosz (1994).

All this suggests that there is little we can learn while comparing available labour and job turnover measures. Job turnover can, at best, provide an upper bound (because some workers can move directly from shrinking to expanding units, in which case their change of jobs would be counted twice by job turnover statistics) for the number of worker reallocations involved by measured changes in the distribution of jobs across business units. And labour turnover can be at best considered an upper bound for the amount of worker reallocations involved by registered hirings and separations insofar as LT, like JT, double-counts job-to-job shifts. The difference between LT and JT is therefore some combination of job-to-job shifts and shifts of workers across establishments that have not altered the size of their workforce from one observation date to another, rather than simply a measure of job-to-job shifts. And this difference is quite large. The few studies presenting JT and LT statistics coming from the same source[26] show that JT is between one-third and one-half of LT.

Job turnover would be a less ambiguous concept for the purpose of worker flows analysis if it captured alterations in the allocation of employment across business units having a strong degree of persistence. In the latter case, JT could be considered as a measure worker reallocations induced by changes in the distribution of employment opportunities, rather than by (supposedly voluntary) shifts of workers across a given set of jobs. In other words, job turnover could be considered as essentially a labour demand concept capturing shifts in the allocation of employment opportunities rather than voluntary decisions of workers to move from one job to another or to leave and enter the world of work. The issue is that both gross job creation and destruction seem to have a large transient component. Davis and Haltiwanger, for instance, show that in the US only about 40 per cent of jobs created in one quarter are still in existence three quarters later and almost half of the jobs destroyed in a quarter are regained three quarters later[27]. Moreover, evidence on patterns of autocorrelation of growth rates of individual business units point to forms of overshooting in employment levels: first-order correlations are negative while growth rates are not correlated more than one year apart [Leonard, 1988; Boeri and Cramer, 1992]. One year of growth tends to be followed by one year of decline in the number of jobs in any establishment. Conversely, if an establishment shrinks, it probably grew in the recent past. The presence of a large transient component in JT may suggest that voluntary quits and other supply-driven forms of worker reallocation are an important determinant of changes in the size of business units rather than the other way round, e.g., recorded job losses in individual establishments may simply reflect unfilled vacancies after the departure of the former post-holder, rather than actual cancellations of positions.

26 See Boeri (1994) for JT and LT data on western Germany, Cassuti, Dell'Aringa and Lucifora (1994) for evidence on Italy, Anderson and Meyer (1994) for comparisons of job and worker turnover rates in the US and Hamermesh, Hassink and van Ours for data on the Netherlands.

27 See Davis and Haltiwanger (1992), ibidem.

Directions for Further Research

In conclusion, there is little loss in not being able of identifying clearcut cyclical properties of job turnover because inferences on labour flows based on available measures of job turnover are, in any event, questionable.

Fortunately, new datasets are currently being developed in many countries that not only allow for tracking over time establishment-level employment changes, but also permit to identify individual workers within each plant. Such datasets have been used so far mainly to disentangle firm and individual characteristics in the estimation of earning functions [Abowd, Kramarz and Margolis, 1994] and to investigate the effects of technologies on the earnings and educational attainments of the workforce [Doms, Dunne and Trotske, 1994]. The potential of these datasets in generating a new wave of empirical research on asymmetries in the cyclical behaviour of job versus worker flows is still largely unexploited. Among the issues which could be possibly addressed drawing on data linking both individuals and establishments: is the time-variation in job destruction dominated by the dynamics of quits or involuntary separations? Do changes over time in job creation rates predominantly reflect changes in the magnitude of job-to-job shifts or other labour market flows?

While available measures of job turnover are not suitable to disentangle the various sources of labour turnover, they are very valuable in analysing the distribution of employment changes. Job turnover is essentially a measure of the dispersion of establishment level growth-rates[1]. Job turnover rates of the order of 20-30 per cent are an indication that establishment level outcomes are highly heterogeneous. Furthermore, a large dispersion of establishment growth rates seems to persist even when the focus in on specific sectors or industries. A common finding of empirical research on job turnover is that the variance of establishment-level employment changes within any industry is by and large the main component of job turnover [Boeri, 1994; Davis and Haltiwanger, 1992; Konings, 1993; Leonard, 1988; Violante and Prat, 1992]. Insofar as this heterogeneity plays an important role in the time variation of gross job flows [Davis and Haltiwanger, 1990; Boeri and Cramer, 1992], the identification of the main sources of this heterogeneity can also shed some light on the determinants of aggregate outcomes.

1 As shown in Boeri (1994), for all establishments having some positive employment at t, job turnover can also be rewritten as:

$$JT_t = \sum_i | g_{it} | s_{it}$$

where g_i and s_{it} denote, respectively, the (employment) growth rate and the (employment) share of establishment i. In other words, job turnover can also be interpreted as a weighted (by the respective employment share) sum of deviations of establishment growth rates from zero.

Regrettably, little attention has been devoted so far by theoretical work to providing explanations for this tremendous heterogeneity of establishment level employment dynamics. Understanding the sources of this heterogeneity remains therefore a main challenge for further work exploiting the rich data sources described in this study.

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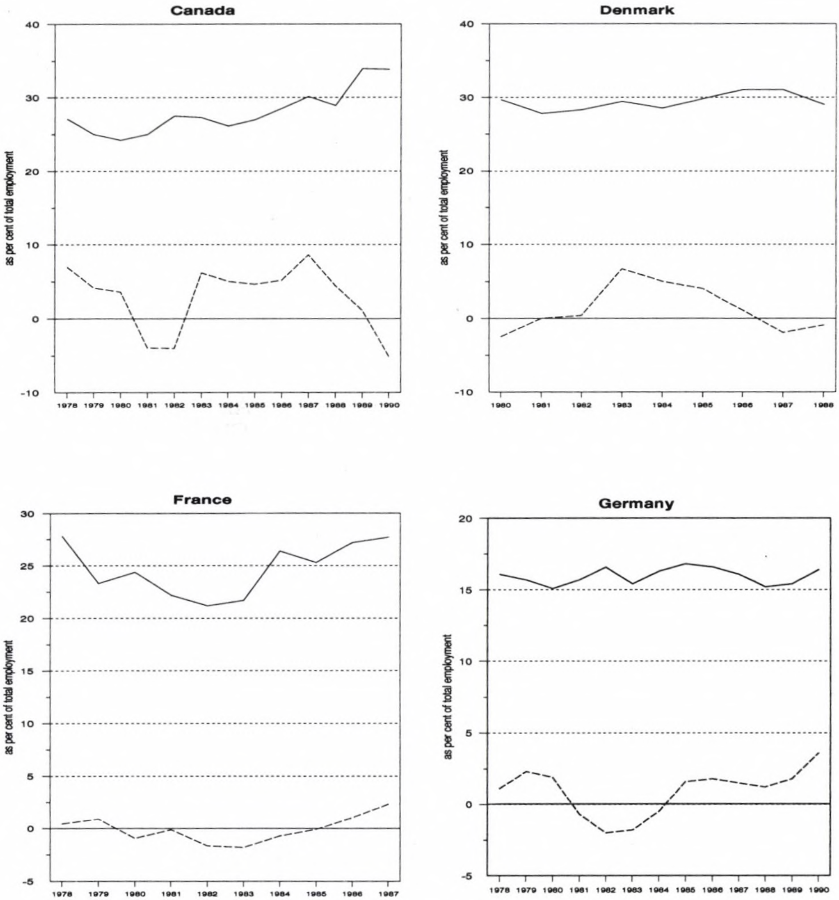
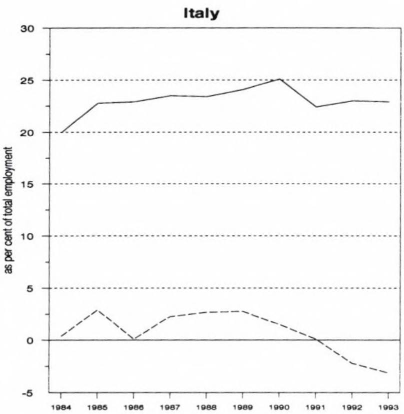
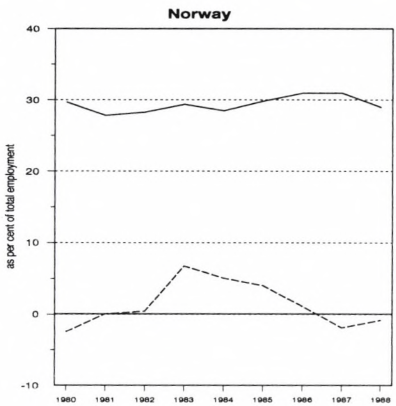
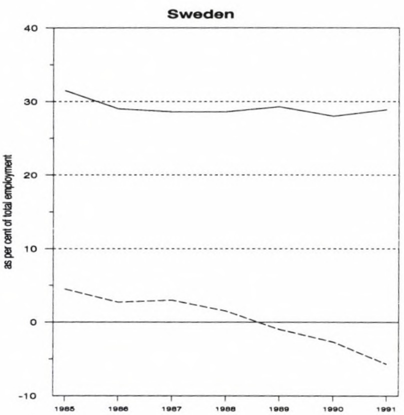
Chart 1
Gross and Net Job Reallocation

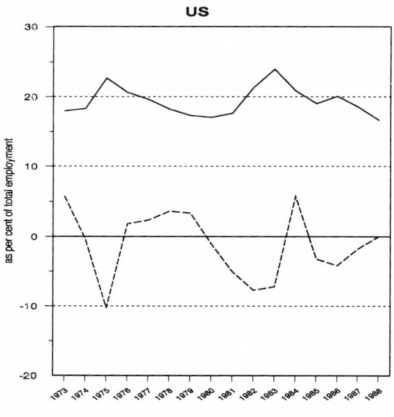
--- NET — JT




Italy
as per cent of total employment
30
25
20
15
10
5
0
-5
1984
1985
1986
1987
1988
1989
1990
1991
1992
1993


Norway
as per cent of total employment
40
30
20
10
0
-10
1980
1981
1982
1983
1984
1985
1986
1987
1988


Sweden
as per cent of total employment
40
30
20
10
0
-10
1985
1986
1987
1988
1989
1990
1991


US
as per cent of total employment
30
20
10
0
-10
-20

Annex 1 -- Data Sources

Canada

Data come from the Small Business database maintained by Statistics Canada based on administrative data (tax form filled annually by employers) providing information on the total wage bill of private enterprises (excluding the self-employed). Estimates of employment in each unit are obtained by dividing data on the wage bill in each enterprise by annual average earnings per employee of the same industry and province, the latter data drawn from Canada's monthly establishment survey. Clearly, these employment estimates may seriously underestimate the actual number of workers in all sectors where part-time employment is especially relevant.

Other limitations of data concern the treatment of plants that have been closed down in the course of the year. By measuring employment in these enterprises in terms of full-year equivalent person-years, the risk is to seriously undersetimate employment in closing units as the wage bill may refer only to part of the year.

Denmark

Data are collected by the Danish Statistical Office (Danmarks Statistiks) on the basis of various administrative sources (tax forms, unemployment register data and population registers). Insofar as identifiers are provided for both business units and workers, it is possible, in principle (this information was not available to the author) to analyse not only job turnover, but also labour turnover.

As individual jobs within any plant can be identified, establishments can be discriminated not only on the basis of their physical location or ownership, but also based on their workforce, which allows for a better identification of true "births" and "deaths". In particular, establishment identifiers would not change even when the plant has changed owner and location, but at least 30 per cent of the workforce is still employed.

France

Data are drawn from the Unemployment Insurance records of the "Union pour l'Emploi dans l'Industrie et le Commerce" (UNEDIC) where all private employers with at least one dependent employee are compelled to register. Individual establishments (not firms!) are assigned identification numbers which makes it possible to follow plant-level employment histories over time.

UNEDIC data seem to cover about 70 per cent of total dependent employment in France the remaining 30% consisting of civil servants and self-employed. The coverage of UNEDIC data is particularly high in manufacturing and in traded services.

Germany

Data are drawn from the Employment Statistics register of the Federal Office of Labour (Bundesanstalt für Arbeit), and collected via the Social Insurance procedure introduced in 1973 that compels employers to report every year all changes occurred in the number of workers who are subject to a health or unemployment insurance or who are participating in a pension scheme. There are legal sanctions for misreporting.

As shown by comparisons with Microzensus data, the register covers all employees in the private sector, i.e., almost 80 per cent of total employment in Germany, the remaining 20% consisting of civil servants and self-employed. Individual plants are assigned separate identification numbers even when they belong to the same firm: this makes it possible to trace individual histories of about 2.7 million establishments which have been operating between 1977 and 1990.

Italy

Data are drawn from records of social security contributions collected by INPS (Instituto Nazionale Previdenza Sociale). Each private enterprise with more than one employee is compelled to declare the total number of dependent workers and the total wage bill. There are legal sanctions for misreporting.

Each employer is assigned by INPS an identification number. By exploiting these codes, a longitudinal data base has been developed which allows to trace individual histories in principle of all private enterprises operating in manufacturing and services (separate records are kept for agricultural workers) and with at least one dependent employee.

Norway

Data are drawn from the Central Register of Establishments and Enterprises maintained by the Central Bureau of Statistics (Statistisk Sentralbyra) on the basis of tax forms and social security records. The statistical unit is the establishment or physical plant, as opposed to the firm. Data on average yearly (rather than on end-of-the year stocks) dependent employment in each unit is provided.

Data used in this study concern only manufacturing, mining and quarrying.

Sweden

As in the case of Norway, data are drawn from the Business Register maintained by Statistics Sweden on the basis of information provided by administrative records (social security records and tax forms). Individual identifiers are provided for both the establishment and the firm. For each unit information is available on the total number of dependent employees.

Data cover private employment in all industries excluding agriculture, forestry and fishing.

US

Data are drawn from the Longitudinal Research Database (LRD) maintained by the Center for Economic Studies at the U.S. Bureau of the Census. The LRD links over time observations on a sub-sample of the Annual Survey of Manufacturing (ASM). The panel on which the ASM is based is changed two years after each Census year as the latter is used as sampling frame. Each year, however, new establishments are added to the panel to preserve its representative character.

LRD data encompasses US manufacturing establishments with more than 5 employees. Moreover, while all establishments with more than 250 employees are included in the ASM sample, smaller establishments are sampled with probabilities proportional to a measure of size determined for each establishment from the previous Census.

Following Blanchard and Diamond (1989), we assume that each year a fraction δ of the total number of jobs becomes obsolete (e.g., because of the rate of obsolence of machines workers are matched to) and a fraction γ of the total number of idle positions gets profitable. Workers on obsolete jobs cannot be immediately relocated to new posts, but have to experience an intervening unemployment spell. As we are interested in the cyclical properties of job creation and destruction, we focus on aggregate shocks[1] only. In particular, we assume that δ and γ are, respectively, non-increasing and non-decreasing functions of an aggregate (demand) shock, Y, i.e., that:

$$\delta = g(Y) \qquad g' \leq 0 \tag{1}$$

and:

$$\gamma = f(Y) \qquad f' \geq 0 \tag{2}$$

The aggregate shock evolves as a first-order (exogenous) Markov process satisfying:

$$\delta F / \delta Y < 0 \tag{3}$$

where F is the transition distribution function:

$$F(\tilde{Y}/Y) = Pr(Y_t \leq \tilde{Y}/Y_{t-1} = Y)$$

that, is, aggregate shocks exhibit some degree of persistence, in the sense of first-

[1] Allowing for idyosincratic shocks is a simple extension of this model insofar as aggregate and plant-specific shocks are not correlated.

order stochastic dominance[2]. We are interested in analysing the effects of employment security regulations on job turnover. Hence, we will assume that employers can layoff workers with one period notice[3] and incurring in (quadratic) adjustment cost:

$$c_i = b s_i^2 \tag{5}$$

where b is a parameter and s_i denotes the total number of separations in the firm:

$$s_{it} = -I(x_{it} - x_{it-1}) - q_{it-1} \tag{6}$$

where q stands for voluntary quits and I is an indicator function that takes the value 1 when x_i is lower or equal than x_{i-1} and 0 otherwise. Since our data mainly capture separations initiated by the employers, we will further assume -- along with most of the job-matching literature -- that $q_i = 0$ for all firms. Employers decide upon planned layoffs to be implemented next period after having observed this period Y. As shown by Hamermesh (1993), the expected profit maximisation problem of the firm leads to the dynamic labour demand equation:

$$s_{it} = \lambda s^{*}_{it-1} = \lambda \varphi(Y_{t-1}) x_{it-1} \qquad \varphi^{\prime} < 0 \tag{7}$$

where $0 < \lambda < 1$ is an implicit function decreasing in b and the latter inequality holds because of the property of the function g and of the stochastic process in Y. Aggregating over firms, we obtain then the overall job destruction rate:

[2] Some persistence in shocks is required to ensure that firms reduce their workforce in response to negative shocks in presence of quadratic firing costs and compulsory advance notification of dismissals.

[3] This corresponds to practice in most OECD countries where advance notification of dismissals is required. The implications of allowing for instantaneous dismissals are, however, discussed below.

$$NEG_t = \lambda\varphi(Y_{t-1})(1-U_{t-1}) \tag{8}$$

The matching of jobseekers and vacancies is time-consuming and can be described by the aggregate matching function:

$$POS_t = M(U_{t-1}, V_{t-1}) \qquad M_u, M_v \geq 0 \qquad M_{uu}, M_{vv} < 0 \tag{9}$$

In order to close the model, we need to specify the law of motion of vacancies. As suggested by equation (2), new vacancies are posted at a rate γ (non-decreasing in the aggregate shock) of the stock of unprofitable jobs (idle capacity). Vacancies posted at time t can then be filled next period via the matching technology outlined above, i.e.:

$$\Delta V_t = \gamma(U_{t-1} - V_{t-1}) - M(U_{t-1}, V_{t-1}) \tag{10}$$

As the labour force is fixed, the dynamics of unemployment is fully characterised by job creation and destruction rates, defined, respectively, by (8) and (9):

$$\Delta U_t = \lambda\varphi(1-U_{t-1}) - M(U_{t-1}, V_{t-1}) \tag{11}$$

We consider first, the comparative statics effects of the tightening of employment security regulations at the steady state equilibrium. Chart 1 displays the vacancy (VV) and the Beveridge (UV) curves obtained, respectively, by equating (10) and (11) to zero. The UV curve slopes downwards[4] insofar as a rise in U (involving

[4] Formally, by implicit function rule, the slope of the UV curve is given by:

$$\frac{\delta V}{\delta U} = -\frac{(\lambda\varphi + M_u)}{M_v}$$

which is negative insofar as the matching function is increasing in both its arguments and λ and δ are positive. The slope of the VV curve is given by:

a decline in inflow rates per given realisations of the aggregate shock) has to be compensated by a decline in the number of posted vacancies in order to maintain unemployment at its long-run equilibrium. The VV curve is upward sloping because a rise in U involves an increase in the number of idle positions, and hence, for given realizations of the aggregate shock, larger vacancy inflows. This has to be compensated by an increase in the number of filled positions, and given the matching technology, by larger stocks of vacancies. The steady state equilibrium lying at the intersection of the two curves is globally stable.

The effects of reductions in λ (induced by the tightening of employment security schemes) are also displayed in Chart 1. These involve a shift towards the origin of the UV curve whilst the VV curve is unaffected by the change. Thus, the new steady state equilibrium involves less unemployment and vacancies. By the job creation condition and the fact that at the equilibrium POS must equal NEG it then follows that *the long-run job turnover rate* (the sum of POS and NEG) *must decline as a result of the tightening of employment security schemes.*

Two further testable implications of the model can be established, in terms of the elasticities of POS and NEG with respect to the aggregate shock. By (8) we have that:

$$NEG_{Y_{t-1}} = \lambda \varphi'(1-U_{-1}) \leq 0 \tag{12}$$

which shows that *a tightening of employment security regulations reduces the responsiveness of job destruction to aggregate demand shocks.* By (9) and (10), we also have that:

---

which is unambiguously positive. If we allow for instantaneous layoffs in response to aggregate shocks, the slope of the VV curve would be given by:

$$\frac{\delta V}{\delta U} = \frac{\gamma - M_u}{\gamma + M_v}$$

which is positive insofar as γ exceeds Mu over the relevant range. The case where VV slopes downwards or bends after reaching a critical unemployment level (with the consequent possibility of multiple equilibria) is not discussed herein for simplicity. Note, however, that this case would not affect our results insofar as the "bad" equilibrium is located in a region where M_v exceeds M_u.

$$POS_{Y_{t-1}} = \eta_{M,V}\, O_V\, \gamma'\, (U_{t-2} - V_{t-2}) \tag{13}$$

where O_V stand for the vacancy outflow rate and η_{MV} denotes the elasticity of job matching with respect to vacancies. Equation (13) is positive by (2). In the case where layoffs are instantaneous, the responsiveness of POS to cyclical fluctuations in demand would be given by:

$$POS_{Y_{t-1}} = \eta_{M,U}\, O_U\, NEG_{Y_{t-1}} + \eta_{M,V}\, O_V\, \gamma'\, (U_{t-2} - V_{t-2}) \tag{13'}$$

where Ou stands for the outflow rate from unemployment and η_{MU} for the elasticity of job finds with respect to the unemployment stock. Equation (13') suggests that -- under instantaneous layoff policies of firms -- the response of job creation to aggregate shocks is a weighted (by the correspondent matching technology elasticities) sum of the effects of the shock on unemployment inflows and on vacancy formation. The first effect is negative insofar as a positive shocks involves lower inflows into unemployment and therefore less matching next period, while the second is positive because of the effects of the shock on vacancy formation and hence on job-matching.

Under (13) changes in employment security regulations do not affect POSy, while under (13') the impact of policy changes is ambiguous[5]. However, in both cases *the tightening of employment security regulations tends to increase the cyclical sensitivity of gross job creation relative to that of job destruction.* In fact, under (13'):

[5] In fact, under (13'):

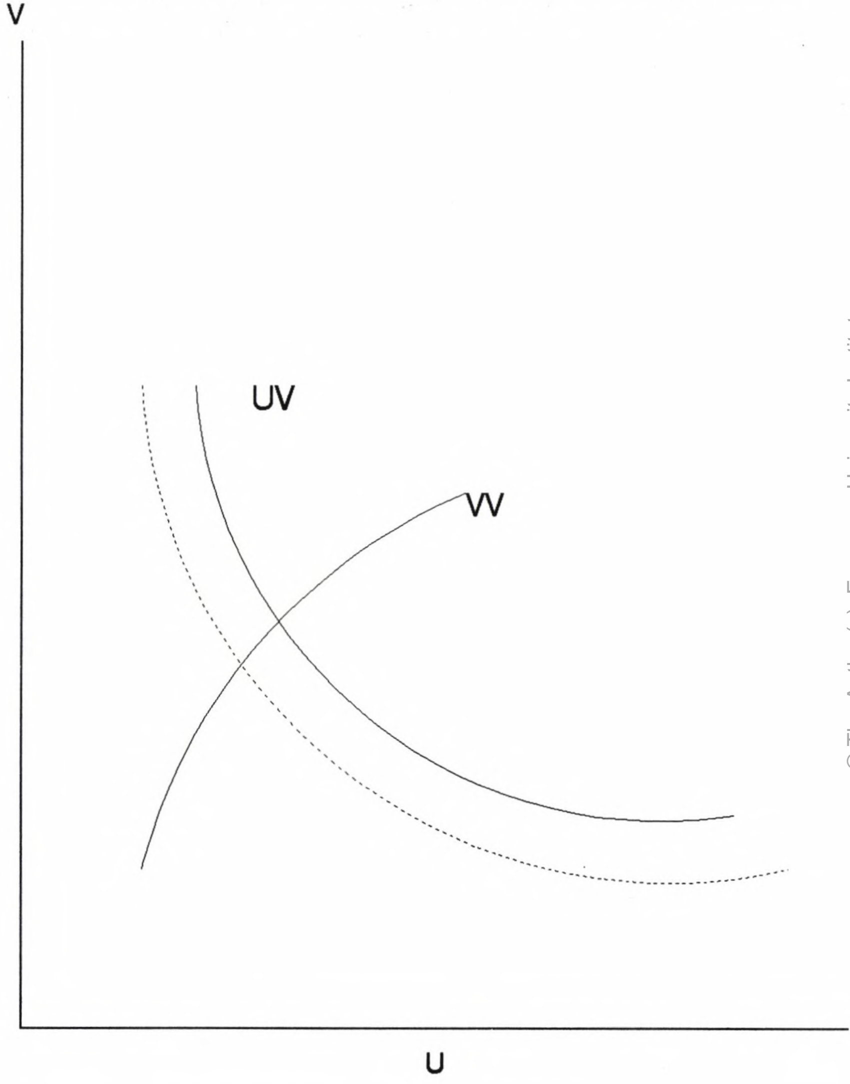
$$POS_{Y\lambda} = M_{UU}\, NEG_Y - M_U\, NEG_{Y\lambda}$$

where the first term is positive by (12) and the convexity of matching technologies, while the second is positive.

$$\frac{POS_Y}{NEG_Y} = -\frac{\eta_{OU}}{\eta_{OV}}\frac{V}{U} + \frac{\gamma'}{|NEG_Y|} \qquad (14)$$

and by (12) we know that the cyclical sensitivity of job destruction is negatively affected by the tightening of employment security regulations. These results imply that *the responsiveness of net flows (NET, the difference between POS and NEG) is accrued when employment security regulations become less stringent.*

Chart 2
Comparative Statics at the Steady State



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